

the apex. Mesopleuræ coarsely rugosely punctured. Metapleuræ longitudinally striated, the striæ weaker at the base. Wings fuscous violaceous, the hinder pair paler than the anterior; the stigma and nervures dark fuscous; the third transverse cubital nervure is interstitial, with the nervure bounding the top of the radial cellule; the latter is clearly separated from the radius. Legs thickly covered with white hairs; those on the tarsi have a fulvous hue. Abdomen shining, sparsely minutely punctured; the pygidium is more strongly punctured, except on the apex; the ventral surface is sparsely covered with white hair.

A distinct species, not nearly allied to any of those already described. It is one of the largest species.

XIII.—*Some Notes on Nomenclature* *.

By FREDERICK PICKARD CAMBRIDGE, B.A., F.Z.S.

IN an ordinary way it would not be necessary to make any reply to Dr. Dahl's "One Word more on the International Rules of Nomenclature" (Zool. Anzeiger, Bd. xxv. Feb. 1902), for he considers this to be the last word. Since, however, he asks for some answer to his questions, and has, as he says, taken up his pen for my special enlightenment, it would be uncourteous were I to refuse to return the compliment.

In the first place, Dahl complains that I am upsetting the unanimity which has hitherto prevailed as to the types of genera, and quotes *Gnaphosa*, *Micromata*, and *Salticus* as instances. In the case of the last genus, however, Simon, in the latest part of his Hist. Nat. Araignées, gives *scenicus* as the type; whereas Thorell and nearly all authors, including Simon himself, have hitherto regarded *formicarius* as the type. I might add a few more instances. Thorell gave *sisyphium*, Clerck, as the type of *Theridion*; Simon gives *redimitum*, Linn. Simon restores *Araneus*; Thorell, until quite lately, upheld *Epeira*. Thorell again gives us *lugubris*, Walck., as the type of *Lycosa*; Simon gives us *tarentula*, Rossi. Thorell regarded *mirabilis*, Clerck, as the type of *Ocyale*; Simon gives us *atalanta*, Aud., &c. &c. So much for the prevailing unanimity. But I may be permitted to suggest that Dr. Dahl need not exercise himself over the iniquity of changing a name or two here and there, seeing

* Cf. Ann. & Mag. Nat. Hist. ser. 7, vol. viii. p. 403 (Nov. 1901).

that, in the case of Clerck, the International Rules have upset thirty or forty names concerning which an absolute unanimity has prevailed amongst authors for quite a century. If we are to swallow this camel, I for one shall not strain at the change of a name or so where necessary in the cause of consistency.

I might also point out that in other branches of zoology—Aves, Lepidoptera, Coleoptera, &c.—I cannot now recognize by name many of the oldest friends of my youth. But if the change has been necessary in these cases, there is no reason to object to necessary changes in the case of much less popular groups, such as the Araneæ.

The Value of a Definite Type Species.

On the last page of his paper Dr. Dahl says that he considers that the fixation of a type would be of slight practical utility, and he adds:—"So far as I can see, I can distinguish three cases in which the type comes into question: (1) I entirely agree with the founder as to the extent of his genus; (2) I agree with a later author; (3) I have my own view." And Dahl asks me to point out when I consider that a practical difficulty arises which would be avoided by the selection of a type.

I answer at once, *in every case*, namely the practical difficulty of ascertaining what exactly is the view to begin with. Dahl appears to misunderstand the question at issue. *The practical value of a selected type comes in before we can form any definite view or opinion as to the characters of a genus at all, either to agree with or to differ from.* We are dealing, for instance, with Latreille's genus *Lycosa*, having before us a number of specimens to classify, many of which we may consider might conveniently be denoted by different group names. We do not wish, however, to coin a number of new names before we have definitely determined that there are no names available which would suit our purpose. We wish for some definite criterion by which we may judge as to which of our specimens belong to *Lycosa*, which to *Tarentula*, *Trochosa*, *Pardosa*, *Arctosa*, *Potamia*, *Hogna*, *Diapontia*, *Tricca*, *Alopecosa*, *Trochosina*, *Leena*, &c. &c. This can only be secured by fixing a single type species to each name. We must determine the type species of *Lycosa* first; and of this genus Dahl insists that we must wait for a future worker to break it up before we can settle on the type, and this worker must further make a new genus of the species under *Lycosa* before his selection will be valid, and so too with all other genera where types have not been cited.

We have waited for nearly a hundred years to ascertain what we mean when we use the generic name *Lycosa*, and if we are to make a new genus every time we want to settle the type of an old one, it seems likely that we shall wait for another century.

On account of Inadequate Diagnoses.

In very many cases, moreover, generic diagnoses contain only characters which apply equally well to the whole *family*; and still more noticeable is the fact that there are often plenty of excellent characters on which genera could be based which were never even observed by the authors who founded the divisions, and do not occur in the diagnoses.

As notable instances of this, one may refer to the minute diagnoses of genera and species made by Dr. Thorell in his study of the Scorpiones. It is not possible to tell from those diagnoses even the *family* to which the species described belonged, because the one character on which the division into families can be satisfactorily based was *overlooked*. So, too, in the case of the Aviculariidae. The subfamily divisions are now based upon characters of which, except in two cases, the very existence was not noticed—namely, the organs of stridulation.

So that unless there are definite type-species to refer to, it is not possible to make any further progress in systematic knowledge. We continue to muddle on, checked at every point by inadequate diagnoses. But directly we have a type-species selected for each generic name we are in a position to judge of the value of the divisions indicated by those names, and also of others which we may ourselves contemplate. We can refer to the facts and see for ourselves, and we shall not then be making new generic appellations on the strength of newly-discovered characters, when there are probably plenty of names already available, if we could but examine definite types. We want to classify our material according to the facts we find in Nature, not by the concepts to which we are restricted by a study of the diagnoses printed in literature. I may remark, too, that type-species of genera, unless they happen to be unique, can always be freely inter-exchanged, and thus some progress in the science made.

As a concrete illustration of a practical difficulty arising for the systematic zoologist, I would suggest that anyone who doubts the value of definite type-species should secure a collection of 500 species of the Salticidae, for instance, and endeavour to classify them by the light of original generic

diagnoses or by the views and standpoints of later authors. If he does not soon find himself writing to living authors for their *types* and fixing upon types for those who are dead, I shall be very much surprised.

As to whether I or Dr. Dahl agree with or differ from any other author as to the extent of his genus is of absolutely no importance to anyone. Nor are the views, standpoints, or concepts either of the original or any later author of any importance either. We are, happily or unhappily, dealing with names and the definite characters to be connoted by them, and we want these characters permanently attached to one or other name by means of a single type-species, so that we can all, when we wish, go to the facts themselves and understand what we are talking about when we use these names.

Names are the current coin in the realms of systematic zoology, necessary for the interexchange of ideas as to the facts; but until these names have a fixed and definite character-value, chaos can be the only result of using them.

But Dr. Dahl says "*of course everyone is at liberty to choose a type for his own private purposes.*" But authors do not choose types for their private purposes; their selections usually appear in publications, and thus the confusion begins. Students consult these publications in order to ascertain what conception, for instance, they are to form as to a certain genus. There may be four or five authors dealing with a genus of ten or a dozen species, and the student is often confronted with three or four different conceptions of the same genus. Some authors will have made new genera based on other authors' concepts of the original genus, each one taking a different author's concept. Often, too, an author's original concept of another author's genus will itself change in course of time (as in the case of *Salicis*), and students who have been basing new genera upon this author's original conception find that they have been building on a quick-sand. Other authors, totally unable to arrive at any reasonable conclusion as to the original genus at all, and finding that of later authors not one has the same conception, simply ignore the whole question. They then probably make another new genus, when several already exist which would meet the case, if types had been definitely selected; or, still worse, they further increase the confusion by adding to literature yet another concept of the original genus of their own. What we need is a definite type-species for each generic name, so that everyone can go to the fact and

find out from it what are the characters which are connoted by that name.

That in which Thorell in *Europ. Spid.* and Simon in *Hist. Nat. Ar.* have made so excellent a beginning by selecting types for genera has to be continued, taking care that all types are fixed by one and the same definite process, so that errors in the various steps can be corrected, and the whole science placed upon some solid and definite basis.

The Selected Type.

When we have made up our minds that *type-species* are a necessity in systematic zoology, we shall be in a position to discuss the processes of arriving at this type.

Dr. Dahl quotes three points in which he supposes that the rules I follow differ from those of the International Congress. The third is this: "*Any author, even some time after the establishment of a genus, may fix the type, if the first author has omitted to do so.*" Dahl adds that I seem to assume that the International Rules would be against the process of elimination for the establishment of a definite type. On the contrary, it would never occur to me that any body of rules should contemplate an attitude so illogical, in view of their decision in paragraph 35, that the first author has to ascribe the original name of the genus to that part of it which he considers suitable.

By what *right* do these rules tamper with the original genus at all? Limited in any way, the result cannot be said to be the original author's conception of his genus. And if the exigencies of systematic zoology demand a further limitation to a single species, even an International Congress cannot dispute the right to take that action. But if the International Rules still adhere to the decision that the determination of the author who first breaks up the genus "*cannot afterwards be modified,*" then no *type-species* can ever be fixed upon in cases where more than one species has been left in by the first author's action.

As to the absence of a paragraph on the point, this simply means that the necessity of fixing on a single *type-species* never occurred to those who drew up the rules.

I have, however, never heard any reasonable argument against regarding the first definitely selected *type-species* as "*the type*" to which all may refer when they wish to know what the characters of a genus may be. One might object that a species possibly would be selected which afterwards

was found to be congeneric with the type of a previous genus, and so the name would be lost as a synonym. But the same objection applies to the case of an author originally breaking up the genus and limiting it to one species, when he himself founds a new genus out of one or more of the residue. So that this objection is of no importance.

There are three very good reasons for recognizing the definite selection of a type-species:—

- (1) Pure elimination, where it leaves us with a single species, is liable to leave us, especially in the case of the older genera, with a phantom species, whose identity is not known and probably never will be known, since all the well-known species have been removed to new genera. The recognition of a type-species, definitely selected by the terms *typ*, *typus*, or *type*, enables us to avoid this contingency.
- (2) Where it leaves us with several species, we can never really know what we are talking about, when we refer to a generic name, until the residue has been finally reduced to *one* species.

In the case of *Lycosa* mentioned above, Dahl tells us that the task of settling on a type "remains for the future worker who again breaks up the residue."

This is precisely what I was told twenty years ago, uttered under the auspices of the "International Rules," and we are still awaiting the advent of that future worker who shall inform us, by breaking up the residue left in by elimination and forming a new genus therefrom, what we are to understand when we speak of *Lycosa*, and, I may add, of dozens of other genera left in like predicament.

But we want to know *now* for immediate use, just as we did twenty, fifty, a hundred years ago.

- (3) It has just dawned upon me that I myself may be the "*future worker*." I have before me something like *ten* genera, each consisting of several species, and I wish to fix upon a type for each, because I believe that the species involved may be conveniently broken up into groups distinguishable by a definite name. But, according to the International Rules (sec. Dahl), I must make a new genus out of the residue of each before I can fix upon a type-species. I must found *ten new genera* because I want to fix on the *type* for ten old ones.

But if there is one thing we want to avoid, it is the making of new genera before we know what the old ones are, and

we cannot tell what the old ones are until we have definitely fixed their types.

In the case here mentioned, which is not hypothetical, but constitutes a problem really at this moment before me, I believe that the old names will be quite sufficient to represent the groups I have in view, and one does not want to add a lot of useless names to the literature merely for the sake of justifying the selection of types for those already existing.

If, however, I am compelled to do this, I merely found ten *bogus* genera, of no systematic value whatever, by quoting a name and adding a type species.

If the International Rules are ready to vindicate this action, I am afraid I really cannot endorse their decision.

For these reasons then, and there are probably others which one has not foreseen, I look upon the recognition of a type-species definitely selected by a later author as of the highest value in systematic zoology. The action is simple and direct, it saves trouble, enables us to avoid waiting for future workers, prevents the unnecessary multiplication of names, and sets nomenclature now at once, for our own immediate use, upon a fixed and definite basis.

There still remain two points to be noticed, in which Dr. Dahl supposes that my methods differ from those of the International Congress:—

(1) *That a binominal name established before 1758 is valid.*

Before Dahl wrote this he should have already received my paper in which I said that I was perfectly prepared to agree that Clerck's names be allowed to lapse, so that this supposition is incorrect.

(2) *The earlier page of the same edition of a work has priority over a later page.*

Thus crudely stated, the point involved is open to misconception.

I hold, and I know of no one else who does not, that a name printed on an earlier page of the same edition of a work, or on an earlier line of the same page, must have priority over the later name, when there is *no doubt* that the species denoted by the two names are identical.

If, for instance, the species represented by *Aranea riparia* and *Ar. labyrinthica* be identical, then the first name *must* stand. If the identity of the two species be not certain, then, of course, the name decided upon by the first author who selected it would stand *for the time being*. But if the type specimen of *A. riparia*, Linn., turned up at Burlington House, as might happen, and proved to be what had hitherto been

called *labyrinthica*, then the name *must* be *riparia*, because it occurs earlier on the page than *labyrinthica*.

If this conclusion be contrary to the International Rules, they must be very strange rules.

As to whether, when the type has been lost, the two names are to be regarded as denoting the same species or not, is entirely a matter for mutual discussion between arachnologists, who, if possible, would come to some definite conclusion on the point.

XIV.—*The Morphology of the Madreporaria*.—II. *Increase of Mesenteries in Madrepora beyond the Protocnemic Stage*.

By J. E. DUERDEN, Ph.D., A.R.C.Sc. (Lond.), Bruce Fellow, Johns Hopkins University*.

IN the 'Johns Hopkins University Circulars' (1900), I refer briefly to the discovery that in the genus *Porites* the mesenteries beyond the primary six pairs (protocnemes) are added in bilateral pairs, within the entocoel of either the dorsal or ventral pair of directives. This method of mesenterial increase is shown to differ from that characteristic of recent corals generally. The results are also briefly contrasted with the method of mesenterial increase occurring in the three great divisions of the Actiniaria—Hexactiniae, Zoanthææ, and Cerianthææ. The close resemblance of the polyps of *Madrepora*† to those of *Porites*, with regard to the number and arrangement of the tentacles and mesenteries, suggested that perhaps a similar mode of mesenterial addition might be followed in that genus also, and the present investigation was undertaken to determine this.

In West-Indian waters at least three well-known types of

* From the 'Johns Hopkins University Circulars,' vol. xxi. No. 157, pp. 59-66 (April, 1902). For Part I. see 'Annals' for May 1902, p. 381.

† Since the paper was written I have received Mr. T. Wayland Vaughan's reports, "Some Fossil Corals from the Elevated Reefs of Curaçao, Arube, and Bonaire," and "The Stony Corals of the Portorican Waters," 1901. Following Brook (1893), Vaughan points out that none of the species at present called *Madrepora* were included within this genus by Linnæus in 1758, and that therefore the name cannot be retained for the forms embraced by Dana, Milne-Edwards and Haime, and later authors. Vaughan suggests its replacement by *Isopora*, a term first employed by Studer in 1878 in a subgeneric sense. More recently Prof. A. E. Verrill (1901) has come to the conclusion that the *Aeropora* of Oken (1815) has much better claims for adoption in place of *Madrepora*.